MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXXII. *December* 8, 1871. No. 2.

WILLIAM LASSELL, Esq., F.R.S., President, in the Chair.

Duncan Darrock, Esq, 5 Beaufort Gardens, and
Edwin George Monk, Esq., York,

were balloted for and duly elected Fellows of the Society.

On Telescopic Observations of the Phenomena seen in contiguity with the Moon's Limb during Eclipses of the Sun, and the Results which have been therefrom deduced. By Professor Grant.

The application of the spectroscope to observations of the envelope of red matter encompassing the solar photosphere, an envelope which has been named *the chromosphere*, has given rise to occasional allusions to the anterior telescopic observations of the same phenomenon, as revealed during eclipses of the Sun, and to the results hence deduced. I beg leave to submit to the Society a few remarks on this subject.

Admitting the existence of such an envelope and its thickness to be inconsiderable, the question arises, under what appearance should it present itself during eclipses of the Sun.

Since the apparent diameter of the Moon sometimes exceeds, sometimes equals, and sometimes falls short of, the apparent diameter of the Sun, it is obviously possible that the existence of the envelope may be occasionally revealed by the phenomenon of a circle of red light surrounding the Moon's disk during slightly total eclipses of the Sun. No instance, however, of such an occurrence has been recorded in the annals of telescopic observations of solar eclipses.*

* Secchi supposes the envelope encompassing the solar photosphere to have a thickness included between 10″ and 15″. Let us assume its mean thickness to be 12″·5. Now, since the apparent diameter of the Moon may be equal to, or greater or less, than the apparent diameter of the Sun, it is quite conceivable that a total eclipse of the Sun may occur during which the envelope may be seen

In solar eclipses generally it is plain that the existence of such an envelope may be *partially* revealed by the appearance of an arc of red light at the part of the Moon's limb where it just covers or slightly overlaps the Sun's disk. This partial covering or slight overlapping of the Sun's disk may occur — 1°, during a total eclipse; 2°, during a nearly total eclipse; 3°, during an annular eclipse; 4°, during a nearly annular eclipse. Let us consider each of these cases separately.

1. *Total Eclipses.*—If the observer be situated in the line of central totality the Moon would just cover the solar disk at the instants of immersion and emersion, and there should be visible a corresponding arc of red light contiguous to the Moon's limb, namely, first at the eastern and then at the western limb, having its centre in each case coincident with the point of interior contact of the solar and lunar disks. If the observer proceeds northwards the Moon's centre will be depressed by parallax, and the points of interior contact will gradually recede upon the upper limb, approaching each other and finally merging together at the apex when the observer has reached the northern limit of totality. Similarly, if the observer travel southwards, the points of interior contact will gradually shift upon the lower limb, approaching each other, and finally merging together at the apex when the observer has reached the southern limit of totality.

2. *Nearly Total Eclipses.*—If the observer be situated a little beyond the northern or southern line of totality, the arc of red light may be visible at the part of the upper or lower limb of the Moon where it slightly overlaps the solar disk.

3. *Annular Eclipses.*—The results as regards the shifting of the points of interior contact are here the same as in the case of total eclipses, except that, as the observer proceeds *northwards*, the points of interior contact approach each other on the *lower* limb instead of the *upper*, finally merging at the apex, while a result, the reverse of this, occurs when he travels towards the *southern* limit of the annular phase.

4. *Nearly Annular Eclipses.*—If the observer be situated a little beyond the northern or southern limit of the annular phase, the arc of red light (modified perhaps in aspect by the presence of Sunlight) may be expected to be seen at the part of the Moon's limb where it slightly overlaps the solar disk.

It is manifest then that in total eclipses of the Sun the interior contacts of the Sun and Moon, and the consequent slight over-

in the form of a red circle encompassing the Moon's limb. Such a condition may be realised on the 30th of November, 1872, when, according to the *Nautical Almanac*, there will be a total eclipse of the Sun, the maximum duration of which on the Earth's surface is 47 seconds. In Latitude 35° 6′ S. and 139° 25′ East Longitude the duration on the central line is stated to be 26 seconds, whence it may be inferred that the envelope would be visible, in the form of a complete circle, during about twenty seconds of time. Mr. Hind has remarked that if we assume the tabular values of the Sun and Moon to be exact, this eclipse will be *annular* near the times of beginning and ending on the Earth' surface.

lapping of the Moon beyond the solar disk, may occur at any part of the Moon's limb, being dependent in this respect on the position of the observer, with respect to the line of central totality. Of course the more nearly the apparent diameters of the Sun and Moon approach to an equality, the narrower will be the limits of totality, and the more rapidly will the points of contact shift upon the Moon's limb for any given change in the position of the observer relative to the central line of totality. On the occasion of the total eclipse of 1842 (which cannot be considered a *large* eclipse) the Moon's shadow passed over some of the most densely populated and easily accessible regions of Southern Europe, and experienced observers were stationed at a great number of places ranging between the two extreme limits of the shadow.

The remarks which I have made with respect to the shifting of the points of interior contact in the case of total eclipses of the Sun, are equally applicable to annular eclipses. As regards nearly total eclipses or nearly annular eclipses, the slight overlapping of the Moon will obviously occur either at the upper or the lower limb.

I would remark, further, that in total eclipses of the Sun observations made at the two extreme limits of totality are especially favourable for observing the arc of red light, since the Moon glides tangentially over the arc, and lingers upon it, whereas in the case of the observer being situated in the centre of the shadow, the Moon advances in a direction which is normal with respect to the arc (at its middle point), and, consequently, passes more rapidly over it. The observations made in France at the two opposite limits of totality during the eclipse of 1842 strikingly illustrate this circumstance.

A similar remark is obviously applicable to annular eclipses.

It was thus, by examining all the recorded observations of solar eclipses available to me from the beginning of the seventeenth century down to the annular eclipse of 1847, analysing and arranging them according as they were total, annular, nearly total, or nearly annular, and confronting the results with each other, that I was led, in the winter of 1850–51, to announce the existence of an envelope of matter superposed upon the solar photosphere.* I shall now quote a few of the observations included in this inquiry.

Total Eclipses of the Sun.

Halley, who observed the eclipse of 1715 at London, remarked that two or three seconds before the emersion, a long and very narrow streak of a dusky but strong red light seemed to colour the dark edge of the Moon on the western side where the Sun was just coming out.

I shall cite four observations of the phenomenon made during

* *History of Physical Astronomy*, pp. 358–411. This work was published in separate parts or numbers, in connexion with a republication of the works of the Library of Useful Knowledge. The first number appeared in September 1848, and the work was finally published complete in March 1852.

the eclipse of 1842; those of Schumacher and Radman made near the central line of totality, and those of Bérard and Guérin at the opposite limits. The observations of Schumacher were made at Vienna, which was situated towards *the north* of the central line of totality. A mean of the results derived from the tables of Burckhardt and Damoiseau made the distance of the upper limbs of the Sun and Moon to be 13″·66. Schumacher states that a little before the emersion of the Sun there appeared near the part of the Moon's disk from behind which the first ray of light was about to issue, *a narrow streak of rose-coloured red light*, which extended perhaps over an arc of 70° or 80° along the Moon's limb, and which disappeared with the luminous ring and the rose-coloured mountains as soon as the first ray of the Sun darted forth. The observations of Radman were made at Padua, which was situated towards *the south* of the line of central totality. These observations have an exceptional value in so far as the question under consideration is concerned, Professor Belli, of the University of Padua, having assigned to Radman the task of specially noting the phenomena visible at the Moon's limb immediately after the total immersion of the Sun, and immediately previous to its emersion. Radman states that no sooner was the Sun totally covered than there appeared a very slender streak of a reddish light surrounding somewhat less than half the circumference of the Moon disposed equally on each side of the place where the last ray of the Sun was extinguished. He adds that it soon vanished, but that it reappeared immediately previous to the emersion of the Sun. The observations of Bérard, who was a Captain in the French Marine and a Corresponding Member of the French Institute, were made at Toulon. Any one may convince himself by referring to the track of the Moon's shadow, as laid down in the *Connaissance des Temps* for 1842, that Toulon was situated almost exactly upon the *southern* line of totality. The observation of Captain Bérard is on this ground so important, that I will give his words in the original. He says,—

"Pendant tout le temps de l'éclipse totale on vit au de là du bord de la Lune, près de la region où le Soleil émergea, une bande rouge très mince dentelée irregulièrement ou comme sillonné ça et là de crevasses."

We have here a precise description of the *sierra* so well known to observers of subsequent eclipses. The visibility of the phenomenon during the whole time of obscuration is in perfect accordance with the position of the place of observation at one of the extreme limits of totality.

At Visan, which was very nearly upon the *northern* line of totality, Guérin, a French observer, perceived immediately after the immersion of the Sun, seven or eight very distinct indentations which might have been taken for so many stars of different magnitudes. They were redder, but less brilliant, than *Mars*. If the whole periphery of the Moon's disk had been bordered with similar luminous points it would have presented the appearance of

a box of ebony garnished with rubies. The phenomenon continued visible during the whole time of totality.*

Annular Eclipses.

Maclaurin, who observed the annular eclipse of 1737, states that a little before the formation of the ring, a remarkable point or speck of pale light appeared near the middle of the part of the Moon's limb that was not yet come upon the disk of the Sun, and that a gleam of light more faint than this light seemed to be extended from it to each horn. This phenomenon was seen about fifteen seconds previous to the projection of the whole body of the Moon upon the solar disk. Lord Aberdour, who observed the same eclipse, remarked that a narrow streak of a dusky red light coloured the dark edge of the Moon immediately before the ring was completed, and after it was dissolved. Henderson, who observed the eclipse of 1836 at Edinburgh, states that previous to the formation of the annulus, when the cusps were about 30° or 40° apart, an arc of faint reddish light was seen extending between them. The appearance lasted several seconds.† Dr. Forster, who observed the eclipse of 1847 at Bruges, mentions that there was visible a very remarkable luminous arc or ridge of light, differently coloured from the rest of the Sun, extending along and immediately on the limb of the Moon between the cusps. From the brief notice of this observation it is not very clear whether it related to an annular or a nearly annular eclipse. It would follow from an inspection of the track of the Moon's penumbra, as laid down in the *Connaissance des Temps* for 1847, that Bruges was situated a very little distance beyond the northern limit of annularity.

Nearly Annular Eclipses.

Short, who observed the eclipse of 1748, at Aberdour Castle, where it was very nearly annular (a little beyond the southern limit), states that when the interval between the Moon's cusps did not exceed one-seventh of the Moon's circumference, a brown light was plainly perceived both by Lord Morton‡ and himself to extend from each cusp about one-third of the whole distance between them. He remarked also at the extremity of this brown light (which came from the western cusp) a larger quantity of light than elsewhere. The illustrious Bessel thus describes the appearance presented during the eclipse of 1836, May 15, as observed at the Königsberg Observatory, which was situated a very short distance beyond the northern limit of the annular phase:—"About twenty-five seconds before the nearest approach of the centres of the two bodies, I perceived near the extremity of the upper cusp a luminous point which, without having the brightness of the Sun, was very dis-

* Guérin adds that the luminous points extended over an arc of about the sixth part of the Moon's circumference.

† Edinburgh was situated about midway between the central and northern line of the annular phase. The first annular contact took place at 167° from the vertex towards the west.

‡ A good amateur observer, the same person who, under the name of Lord Aberdour, observed the annular eclipse of 1737, *vide suprà*.

tinctly visible with the powerful heliometer. As the cusps were then approaching each other, I hoped that the annulus was about to form, but this did not happen. With respect to the point which I have just alluded to, it became more luminous. Other similar points appeared which soon united together, and rendered visible all the part of the Moon's limb included between the extremities of the cusps."* Mr. Twining, an American, observed the eclipse of 1838 at New York. This eclipse was annular at Philadelphia, but failed very slightly in being so at the place where Mr. Twining observed it. He states that several minutes before the nearest completion of the ring, the fine cusps of the Sun's unobscured crescent were prolonged by a hair-breadth line of brightness totally diverse in colour and intensity from the Sun's disk. As the cusps approached the lines or threads of light shot round the Moon's edge between them rapidly, till at a certain time they met and joined in one, thus uniting the cusps.

The visibility of the phenomenon during sunshine, as thus exhibited in the case of annular and nearly annular eclipses, is fully confirmed by the observations of the annular eclipse of May 23, 1867, made in Dalmatia, at Barsecienne, a place situated on the northern limit of the annular phase. On that occasion Herr Kiha, an officer of the Austrian naval service, perceived a prominence near the extremity of the upper cusp 14·4 minutes before the formation of the ring, and was able to follow it during a further interval of 14·7 minutes.† The colour (*braünlich gelb*) which he ascribes to the object accords perfectly with the speck of pale light seen by Maclaurin during the annular eclipse of 1737, with the accumulation of brown light at a point intermediate between the cusps noticed by Short during the nearly annular eclipse of 1748, and furthermore with the observations of Bessel and Henderson in 1836, of Twining in 1838, and Forster in 1847.

In every instance, except one or two, it is expressly stated that the arc of red light was seen at the region of immersion or emersion,—in other words, at the region where the Moon's limb just covered, or slightly overlapped, the Sun's disk. In no instance is it stated to have been observed at any other part of the disk. In annular or nearly annular eclipses, the break observed in some instances in the arc extending from the cusps, is accounted for by the somewhat too great overlapping of the Moon, which would be most effective at the point midway between the cusps.

While engaged in this inquiry I endeavoured to show that the phenomena observed during solar eclipses, whether corona, streaks or patches of light, or prominences great or small, *are*

* Secchi, in his recently published work *Le Soleil*, p. 213, remarks: "Bessel, observant une eclipse annulaire à Königsberg, avait vû le bord de la lune environné d'un circle rouge." This statement must not be accepted in a literal sense. The eclipse was *not* annular at Königsberg, although very nearly so, and most assuredly Bessel did not see the margin of the Moon surrounded by a red circle. The account of the observation which I have given is a translation of Bessel's own description of the phenomenon (*Ast. Nach.* vol. xiv. p. 115).

† *Ast. Nach.* No. 1647, p. 228.

all solar phenomena. While the results of observation were either positively in favour of the solar aspect of the case or adverse to the lunar aspect, there did not appear to exist a shadow of a proof in support of the lunar origin of the phenomenon.

Seeing, then, that when a great number of solar eclipses, observed indifferently with respect to the limiting lines of the Moon's shadow or penumbra, are taken into consideration, the slight overlapping of the Moon takes place at all parts of the solar disk, and that wherever this overlapping really does occur, there is visible at the Moon's limb an arc or band of red light, the existence of such a margin of light becomes thus inductively traceable round the entire contour of the solar disk.

It was under the influence of a conviction forced upon me by these considerations that in the winter of 1850–51 I made the following announcement:—

"According to the usual theory of the physical constitution of the Sun, that body consists of an opaque nucleus, surrounded by an atmosphere in which are suspended, at different elevations, two enveloping substances of dissimilar physical properties, the lower envelope being imperfectly luminous, but capable, in a high degree, of reflecting light, while, on the other hand, the upper envelope forms a resplendent canopy of clouds, which are luminous in themselves, and constitute the source of the light diffused in every direction by the Sun. The observations of solar eclipses would seem to indicate that, above the luminiferous envelope, there exists *a stratum of nebulous matter* which is visible only by means of reflected light. Various interesting questions present themselves for solution in connexion with the admitted existence of such a stratum. In the first place, does *this third envelope* exercise an influence in the production of any of the other phenomena which have been disclosed by observations on the physical constitution of the Sun? M. Arago has very ingeniously suggested that the solar spots which exhibit the aspect of a penumbra without a nucleus, may arise from the superposition of masses of this nebulous matter above the luminiferous envelope. Secondly, the question arises, does any relation subsist between this non-luminous substance, which floats in the upper regions of the solar atmosphere, and either of the other two envelopes?

"Sir John Herschel, in his ingenious theory of the physical origin of the solar spots, supposes a perpetual circulation to be kept up in the solar atmosphere, analogous to that which, in the case of the terrestrial atmosphere, produces the phenomenon of the trade-winds. Now, is it not reasonable enough to suppose that such a circulation will have the effect of continually agitating the non-luminous matter constituting the envelope nearest the solid nucleus of the Sun, and throwing up masses of it above the luminiferous surface?

"This view of the subject, while it carries with it considerable probability, obviates the necessity of introducing into the theory of the physical constitution of the Sun, the idea of a third

envelope independent of the two others. Future observations prosecuted more especially with reference to the positions of the solar spots during the occurrence of eclipses of the Sun, can alone be expected to throw light upon this interesting question." (*History of Physical Astronomy*, page 400.)

The passage which I have here quoted contains the announcement of a fact, and an attempt at an explanation of its physical cause. As regards *the fact*, I consider it to have been established by the observations on which it is founded. With respect to *the explanation*, it is a mere surmise which may or may not contain elements of truth.

In referring to this solution which I gave of the question of the band of red light seen during solar eclipses, I am far from maintaining that it is as perfect as it might be. The combination of the valuable results of telescopic observations of solar eclipses made subsequently to the annular eclipse of 1847, with the previously existing stock of results, would unquestionably improve the solution, and a better still might be obtained by a systematic placing of observers in determinate positions with respect to the limits of the track upon the Earth's surface of the Moon's shadow or penumbra; but now observations on a new principle are available which supersede the necessity of any such attempts.

The interest attached to the envelope of red matter encompassing the solar photosphere, and the importance with which it has been recently invested, are almost wholly due to the brilliant results which have been obtained by the application of the spectroscope to observations of the Sun. Still it appears to me to be desirable, in the interest of truth, that the series of carefully executed telescopic observations of solar eclipses anterior to the year 1848 should have accorded to them their due weight, and that their applicability to a legitimate explanation of some of the phenomena seen on such occasions should be fairly stated.

The Observatory,
Glasgow, November 1871.

In connexion with the foregoing remarks I append an extract from an account of my own observations of the total eclipse of the Sun of July 18, 1860, which I communicated to the Astronomer Royal in the autumn of 1860. The observations were made from a position (about twenty miles north of the line of central totality) on the Sierra de Tolonio, a lofty mountain range, a little to the south of Vitoria, overlooking the valley of the Ebro.

"At a distance of 60° to the right of the vertex, I perceived a third prominence. When first seen it resembled a small cone attached to the Moon's limb. As the Moon advanced over the Sun's disk, it gradually increased in dimensions until it finally attained an altitude of about 1½′. In colour and general aspect it bore a close resemblance to the large prominence near the vertex. While I was engaged in examining the last-mentioned promin-

ence I perceived several other very small peaks which had just come into view, and a few seconds afterwards the motion of the Moon disclosed a long stratum uniting them together at their bases, giving to the whole phenomenon the aspect of a *sierra* resting on the Moon's limb, and extending over an arc of about 20°. This continuous succession of prominences was intensely red, resembling in colour the most beautiful vermilion. The light was very much condensed, there being an entire absence of the streaks which characterized the larger prominences. The re-appearance of the Sun was preceded by an excessively narrow band of red light bounding the Moon's limb and connecting itself with the *sierra* previously observed. This long streak of light extended over an arc of about 25° on the Moon's limb. The reappearance of the first ray of the solar light resembled a bright star emerging at the Moon's limb. It occurred at $3^h\ 3^m\ 10^s$." *

On an Universal Equatoreal. By John Browning, Esq.

Wishing to employ the telescope used by Colonel Tennant for photographing the Solar Eclipse of 1868, for the purpose of photographing the next Solar Eclipse in India, it occurred to the Astronomer Royal that it would be a great advantage to have the instrument so made that it might be considered universal, and used in any latitude between the equator and 60° to 70° N. or S. of the equator. When the instrument had been altered so as to fulfil these conditions, I believe to the satisfaction of the Astronomer Royal, I saw that a more efficient instrument might have been made if it had been planned specially for this purpose. I have prepared for exhibition before the Society a model of such an instrument, and I am now making one of $10\frac{1}{4}$-in. diameter, for a gentleman at Sydney.

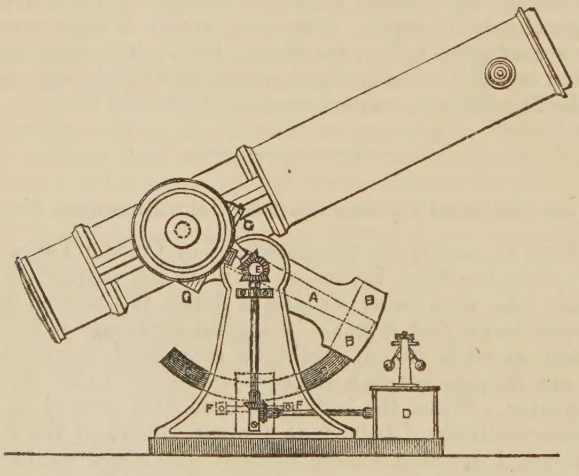
On reference to the diagram, the principle of construction will, I think, be evident.

The whole of the telescope, with the Polar axis, is carried by centres at E; large plates are attached to the sides of a cradle, which carries the Polar axis; AA is the Polar axis; BB the cradle in which it is carried; CC, an arc with a clamping arrangement, of which the adjusting screws are shown at FF; a clamp screw is hidden by the spur-wheels; when this screw is released, the angle of the Polar axis of the instrument may be changed from the horizontal to a nearly vertical position, or let to remain in any intermediate position between these, reclamped and finally adjusted by the capstan-screws FF; D is the clock, which, by means of a bevelled spur-wheel, gives motion to a wheel which revolves round the centre on which the Polar axis turns at E; this wheel, through the intermediation of two other wheels turns the driving-

* Greenwich Mean Time. The observations were made with a Dollond Refractor of $2\frac{3}{4}$ inches aperture.

screw on the hour-circle GG, and thus drives the instrument; as the wheel EE runs loose on the spindle, and the distance between the wheel driven by E, and the main driving-screw on the hour-circle remains unaltered, it is evident that a change in the angle of position of the Polar axis does not interfere so as to prevent the instrument being freely driven by the clock.

As I have before stated, the suggestion that such an instrument should be made, was the Astronomer Royal's; and I have received efficient assistance from Mr. De La Rue in perfecting this contrivance.



On the Motion of Matter projected from the Sun: with special reference to the Outburst witnessed by Prof. Young of America. By Richard A. Proctor, B.A. (Cambridge.)

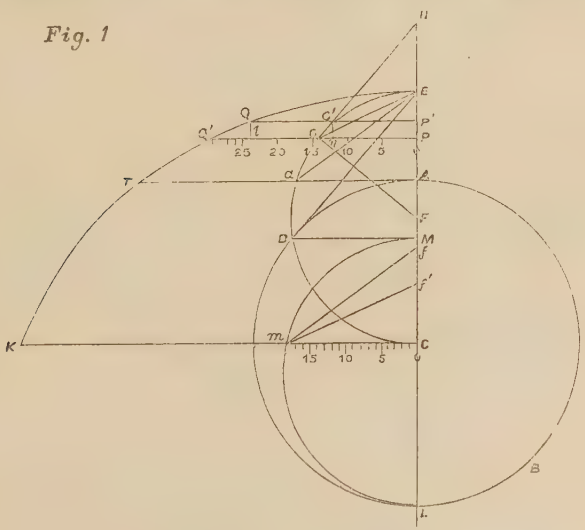
Whatever opinion we may form as to the way in which the matter of certain solar prominences is propelled from beneath the photosphere, there can be little question that such propulsion really takes place. It seems clear indeed that some prominences, more especially those seen in the Sun's polar and equatorial regions, are formed—or rather make their appearance—in the upper regions of the solar atmosphere, and even assume the appearance of eruption prominences by an extension *downwards*, somewhat as a waterspout simulates the appearance of an uprushing column of water though really formed by a descending movement. But it is certain that other prominences are really phenomena of eruption, because Respighi, Zöllner, and Young (probably also Lockyer and Secchi) have been able actually to watch the uprush of the flowing hydrogen.

In the case of any matter thus erupted, we shall clearly obtain

an inferior limit for the value of the initial velocity of outrush, if we assume that the apparent height reached by the matter is the real limit of its upward motion (that is, that there is no foreshortening) and that the solar atmosphere exercises no appreciable influence in retarding the motion. The latter supposition is, however, wholly untenable under the circumstances, while the former must in nearly all cases be erroneous; and I only make these suppositions in order to simplify the subject, noting that their effect is to reduce the estimated velocity of outrush to its lowest limiting value.

We are to deal then, for the present, with the case of matter flung vertically upwards from the Sun's surface and subject only to the influence of solar gravity; I propose to consider the time

Fig. 1



Illustrating the motion of a body descending from rest towards a globe attracting according to the law of nature.

of flight between certain observed levels, not the mere vertical distance attained by the erupted matter; and (as I wish to deal with cases where a great distance from the Sun has been attained) it will be necessary to take into account the different actions of solar gravity at different distances. Zöllner, in dealing with prominences of moderate height, has regarded the solar gravity as constant; but this is evidently not admissible when we come to deal with matter hurled to a height of 200,000 miles, since at that height solar gravity is reduced to less than one-half the value it has at the surface of the Sun.

It is easy to obtain the required formula; and though it is doubtless contained in all treatises on Dynamics, it will be as well to run through the work in this place. In reducing the formula

I have noticed a neat geometrical illustration (and partial proof) which I do not remember to have seen in any book. It not only presents in a striking manner the varying rate at which a body falls towards a centre attracting according to the law of Nature, but it supplies a means whereby the time of flight between any given distances may be readily obtained from a simple construction.

Let C be the centre of a globe A B D, of radius R, and attracting according to the law of nature; let gravity at the surface of the globe be represented by g. Then the attraction exerted at a unit of distance, if the whole mass of the globe were collected at a point, would be $g\,R^2$.

Let a particle falling from rest at E reach the point P in time t; and let A E = H, and C P = x. Then the equation of motion is

$$\frac{d^2x}{dt^2} = -\frac{g\,R^2}{x^2},$$

giving

$$\left(\frac{dx}{dt}\right)^2 = v^2 = \frac{2\,g\,R^2}{x} + C;$$

so that, since the particle starts from rest at a distance R + H from C, we have

$$0 = \frac{2\,g\,R^2}{R+H} + C.$$

For convenience write D for R + H; then we have

$$\left(\frac{dx}{dt}\right)^2 = v^2 = 2\,g\,R^2\left(\frac{1}{x} - \frac{1}{D}\right)$$

$$= \frac{2\,g\,R^2}{D}\left(\frac{D-x}{x}\right) \qquad (1)$$

Thus

$$R\sqrt{\frac{2\,g}{D}} \cdot \frac{dt}{dx} = \frac{x}{\sqrt{D\,x - x^2}}.$$

Integrating we have

$$R\sqrt{\frac{2\,g}{D}} \cdot t = \sqrt{D\,x - x^2} - \frac{D}{2}\cos^{-2}\left(\frac{D-2x}{D}\right) + C$$

But $t = 0$, $x = D$; so that $C = \frac{D\pi}{2}$,

hence we have

$$R\sqrt{\frac{2\,g}{D}}\,t = \sqrt{D\,x - x^2} + \frac{D}{2}\cos^{-1}\left(\frac{2x-D}{D}\right) \quad (2)$$

(where D is equal to the radius of the globe added to the height from which the particle is let fall.)

Equation (1) gives the velocity acquired in falling (from rest) from a height H to a distance x from the centre, and (2) gives the time of falling to that distance. Now the geometrical illustration to which I have referred, relates to the deduction of (2) from (1). We see from (1) that at the point P

$$v^2 = \frac{2g\,R^2}{D}\left(\frac{D-x}{x}\right).$$

Bisect C E in F, and describe the semicircle C D E; then if D E is a tangent to the circle D A B, and if D M is drawn perpendicular to C E,

$$C\,M = \frac{C\,D^2}{C\,E} = \frac{R^2}{D};$$

so that

$$v = \sqrt{2g\,.\,C\,M}\,\sqrt{\frac{P\,E}{C\,P}}. \qquad (\alpha)$$

But if close by G, either on the tangent G H or on the arc G E, we take G′ and draw G′ P′ perpendicular to C E, and G′n perpendicular to G P, we have

$$\frac{G\,G' + G\,n}{P\,P'} = \frac{G\,F + F\,P}{G\,P} = \frac{C\,P}{\sqrt{C\,P\,.\,P\,E}}$$

$$= \sqrt{\frac{C\,P}{P\,E}}.$$

Hence, from (α),

$$\frac{v}{\sqrt{2g\,.\,C\,M}} = \frac{P\,P'}{G\,G' + G\,n},$$

so that

$$\left\{\begin{matrix}\text{the vel.}\\ \text{at P}\end{matrix}\right\} : \left\{\begin{matrix}\text{velocity acquired in falling through}\\ \text{space C M, under const. accel. force } g\end{matrix}\right\}$$

$$:: \left\{\begin{matrix}\text{elem. space}\\ P\,P'\end{matrix}\right\} : \left\{\begin{matrix}\text{sum of elementary}\\ \text{spaces G G' and G } n.\end{matrix}\right\}$$

Therefore the falling particle traverses the space P P′, in the same time that a particle travelling with the velocity acquired in falling through space C M under constant accelerating force g, would traverse the space G G′ + G n. It follows obviously that the time in falling from E to P is the same as would be occupied by a particle in traversing the space (arc E G + G P), with the

velocity acquired in falling through the space C M under a constant accelerating force g. This amounts to saying that

$$t = \frac{\text{P G} + \text{arc G E}}{\sqrt{2g \,.\, \text{C M}}},$$

or that

$$\text{R}\sqrt{\frac{2g}{\text{D}}} \,.\, t = \sqrt{\text{P E} \,.\, \text{P C}} + \text{C F arc G E}$$

$$= \sqrt{(\text{D}-x)x} + \frac{\text{D}}{2}\cos^{-1}\left(\frac{2x-\text{D}}{\text{D}}\right)$$

as before.

The relation here considered affords a very convenient construction for determining times of descent in any given cases. For, if P G be produced to Q so that G Q = arc G E, Q lies on a semi-cycloid K Q C, having C E as diameter; and the relative time of flight from E to any point in A E is at once indicated by drawing through the point an ordinate parallel to C K. The actual time of flight in any given case can also be readily indicated. For let T be the time in which L C would be described with the velocity acquired in falling through a distance equal to L C under accelerating force g; and on L M describe the semicircle L m M; then clearly C m $(= \sqrt{\text{C L} \,.\, \text{C M}})$ will be the space described in time T with the velocity acquired in falling through the space C M under accelerating force g; and we have only to divide C m into parts corresponding to the known time-interval T, and to measure off distances equal to these parts on P Q to find the time of traversing P Q with this uniform velocity, *i.e.*, the time in which the particle falls from E to P. The division in the figure illustrates such measurements in the case of the Sun, the value of T being $18\frac{2}{3}$ minutes.

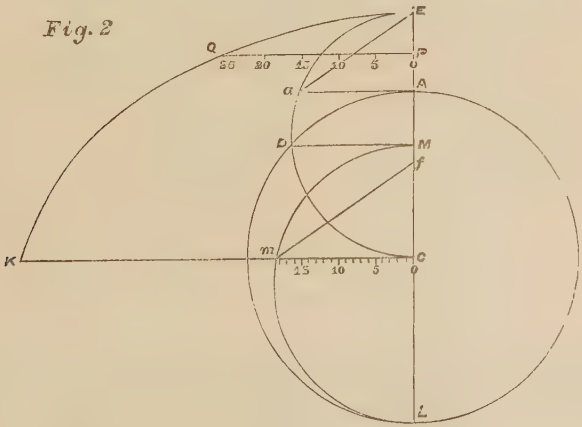
Moreover it is not necessary to construct a cycloid for each case. One carefully constructed cycloid * will serve for all cases, the radius C A being made the geometrical variable. As an instance of the method of construction, I will take Prof. Young's recent remarkable observation of a solar outburst, premising that I only give the constructions as illustrations, and that a proper calculation will follow.

Prof. Young saw wisps of hydrogen carried from a height of 100,000 miles to a height exceeding 200,000 miles from the Sun's surface in ten minutes. It is assumed here that there was no

* It is easy to construct a cycloid with great accuracy, nor need the methods available be here considered. But it is worthy of notice that if a wire coil be formed into a helix having the thread inclined 45° to the axis (which can be readily effected by the method which De Morgan suggested to Admiral Smyth,—see *Bedford Cycle*), and this thread placed on a horizontal table when the Sun is 45° high (the vertical plane including the Sun and the axis of the helix) the coil will throw a cycloidal shadow. (This arrangement is only presented in this way to show exactly how the point of projection should be placed with respect to the helix and plane; of course the projection can be obtained without the Sun's aid.)

foreshortening. The height, 100,000 miles, was determined by estimation; but the ultimate height reached by the hydrogen wisps (that is, the elevation at which they vanished as by a gradual dissolution) results from the mean of three carefully executed and closely accordant measures. This mean was 7′ 49″, corresponding to a height of 210,000 miles. We may safely take 100,000 miles as the vertical range actually traversed, and 200,000 miles as the extreme limit attained. We need not inquire whether the hydrogen wisps were themselves projected from the photosphere,—most probably they were not,—but if not, yet beyond question there was propelled from the Sun some matter which by its own motion caused the hydrogen to traverse the above-mentioned range in the time named. We shall be underrating the velocity of expulsion, in regarding this matter as something solid propelled through a non-resisting medium, and attaining an extreme range of 200,000 miles. What follows will show whether this supposition is admissible.

Fig. 2



Illustrating the construction for determining time of descent of a particle from rest towards a globe attracting according to the law of nature.

Now g for the Sun, with a mile as the unit of length and a second for the unit of time is ·169, and R for the Sun is 425,000. Thus the velocity acquired in traversing R under uniform force g

$$= \sqrt{2g \cdot R}$$

$$= \sqrt{338 \times 425}$$

$$= 379, \text{ very nearly}$$

(this is also the velocity acquired under the Sun's actual attraction by a body moving from an infinite distance to the Sun's surface.)

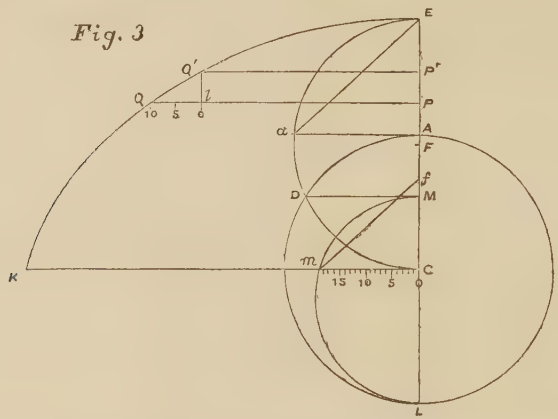
And a distance 425,000 would be traversed with this velocity in $18^m\ 40^s$ (= T.)

Let K Q E, *fig.* 2, be our semi-cycloid (available for many successive constructions if these be only pencilled), and C D E half the generating circle.

Then the following is our construction:—Divide E C into $6\frac{1}{4}$ equal portions, and let E P, P A be two of these parts, so that E A represents 200,000 miles and C A 425,000 miles (the Sun's radius). Describe the semicircle A D L about the centre C and draw D M perpendicular to E C; describe half circle M *m* L. Then *m* C represents T where the ordinate P Q represents time of falling from E to P.

T = $18^h\ 50^m$, and P Q (carefully measured) is found to correspond to about twenty-six minutes.

Fig. 3



Illustrating the construction for determining time of descent between given levels, when a body descends from rest at a given height towards a globe attracting according to the law of nature.

Thus a body propelled upwards from A to E would traverse the distance P E in twenty-six minutes. But the hydrogen wisps watched by Prof. Young traversed the distance represented by P E in ten minutes. Hence either E was not the true limit of their upward motion, or they were retarded by the resistance of the solar atmosphere. Of course if their actual flight was to any extent foreshortened, we should only the more obviously be forced to adopt one or other of those conclusions.

But now let us suppose that the former is the correct solution; and let us inquire what change in the estimated limit of the uprush will give ten minutes as the time of moving (without resistance) from a height of 100,000 to a height of 200,000 miles. Here we shall feel the advantage of the constructive method; for to test the matter by calculation would be a long process, whereas each construction can be completed in a few minutes.

Let us try 375,000 miles as the vertical range. This gives C E = 800,000 miles, and our construction assumes the following appearance. We have A C to represent 425,000; A P = P P′ = 100,000 miles; and Q *l* or (P Q −P′ Q′) to represent the time of flight from P to P′.

The semicircles A D L, M *m* L give us *m* C to represent T or $18^h\ 50^m$; and Q L carefully measured is found to correspond to rather less than ten minutes. It is, however, near enough for our purpose.

It appears, then, that if we set aside the probability, or rather the certainty, that the Sun's atmosphere exerts a retarding influence, we must infer that the matter projected from the Sun reached a height of 375,000 miles, or thereabouts. This implies an initial velocity of about 265 miles per second.*

But it will be well to make an exact calculation,—not that any very great nicety of calculation is really required, but —to illustrate the method to be employed in such cases, as well as to confirm the accuracy of the above constructions.

In equation (2) put $\sqrt{2gR}=379$; R = 425,000; D = 625,000; and x = 525,000; values corresponding to Prof. Young's observations. It thus becomes—

$$\sqrt{\frac{425}{625}}\,(379)\,t = \sqrt{(100{,}000)\,(525{,}000)} + 312{,}500\,\cos^{-1}\left(\frac{1050-625}{625}\right),$$

or

$$379\sqrt{17}\,.\,t = 250{,}000\sqrt{21} + 1{,}562{,}500\,\cos^{-1}\left(\frac{17}{25}\right),$$

$$1562{\cdot}7\,t = 1{,}145{,}100 + 1{,}285{,}800 = 2{,}430{,}900$$

$$t = 1556^s = 25^m\ 56^s.$$

This then is the time which would have been occupied in the flight of matter from a height of 100,000 to a height of 200,000 miles, if the latter height had been the limit of vertical propulsion in a non-resisting medium.

In order to deduce the time of flight t' between the same levels, for the case where the total vertical range is 375,000 miles, we have, putting t_1 for time of *fall* to 200,000 miles above Sun's surface, and t_2 for time of fall to 100,000 miles, the equation,

* This value is of course deduced directly from (1); but it is worthy of notice that it can be deduced at once from *fig.* 3, by drawing A *a* parallel to K C, and *m f* parallel to *a* E; then C *f* represents the required velocity, C L representing 379 miles per second. A similar construction will give the velocity at P, P′ &c. Applied to *fig.* 1, it gives C *f* to represent the velocity at A, C *f*′ to represent the velocity at P; *m f* and *m f*′ being parallel to *a* E and G E respectively. Applied to the case dealt with in *fig.* 2, we get C *f* to represent the velocity at A, where E is the limit of flight; C *f* is found to be rather more than ⅞ of C L; so that the velocity at A is rather more than 210 miles per second.

$$\sqrt{\frac{425}{800}}\,(379)\,t_1 = \sqrt{(175{,}000)\,(625{,}000)} + (400{,}000)\cos^{-1}\left(\frac{125-80}{80}\right),$$

and

$$\sqrt{\frac{425}{800}}\,(379)\,t_2 = \sqrt{(275{,}000)\,(525{,}000)} + (400{,}000)\cos^{-1}\left(\frac{105-80}{80}\right),$$

giving

(since $t_2 - t_1 = t'$)

$$\sqrt{\frac{425}{800}}\,(379)\,t' = 25{,}000\left\{\sqrt{11\times 21} - \sqrt{7\times 25}\right\} + 400{,}000\left\{\cos^{-1}\left(\frac{5}{16}\right) - \cos^{-1}\left(\frac{9}{16}\right)\right\}$$

$$276{\cdot}25\,t' = 49{,}250 + 111{,}816 = 161{,}066,$$

$$t' = 583^{s} = 9^{m}\,43^{s}.$$

This is very near to Prof. Young's ten minutes. I had found that an extreme height of 400,000 miles gave $9^{m}\,24^{s}$ for the time of flight between vertical altitudes 100,000 miles and 200,000 miles. It will be found that a height of 360,000 miles gives $9^{m}{\cdot}585$, which is sufficiently near to Prof. Young's time.

Now to attain a height of 360,000 miles a projectile from the Sun's surface must have an initial velocity

$$= \sqrt{2gR}\cdot\sqrt{\frac{360{,}000}{785{,}000}} = 379\sqrt{\frac{72}{157}}$$

$$= 257 \text{ miles per second.}$$

The eruptive velocity, then, at the Sun's surface, cannot possibly have been less than this. When we consider, however, that the observed uprushing matter was vapourous, and not very greatly compressed (for otherwise the spectrum of the hydrogen would have been continuous and the spectroscope would have given no indications of the phenomenon), we cannot but believe that the resisting action of the solar atmosphere must have enormously reduced the velocity of uprush before a height of 100,000 miles was attained, as well as during the observed motion to the height of 200,000 miles. It would be safer indeed to assume that the initial velocity was a considerable multiple of the above-mentioned velocity, than only in excess of it by some moderate proportion. Those who are acquainted with the action of our own

atmosphere on the flight of cannon-balls (whereby the range becomes a mere fraction of that due to the velocity of propulsion), will be ready to admit that hydrogen rushing through 100,000 miles even of a rare atmosphere, with a velocity so great as to leave a residue sufficient to carry the hydrogen 100,000 miles in the next ten minutes, must have been propelled from the Sun's surface with a velocity many times exceeding 257 miles per second, the result calculated for an unresisted projectile. Nor need we wonder that the spectroscope supplies no evidence of such velocities, since when motions so rapid existed, others of all degrees of rapidity down to such comparatively moderate velocities as twenty or thirty miles per second would also exist, and the spectral lines of the hydrogen so moving would be too greatly widened to be discerned.

Now the point to be specially noticed is, that supposing matter more condensed than the upflung hydrogen to be propelled from the Sun during these eruptions, such matter would retain a much larger proportion of the velocity originally imparted. Setting the velocity of outrush, in the case we have been considering, at only twice the amount deduced on the hypothesis of no resistance (and it is incredible that the proportion can be so small), we have a velocity of projection of more than 500 miles per second; and if the more condensed erupted matter retained but that portion of its velocity corresponding to three-fourths of this initial velocity (which may fairly be admitted when we are supposing the hydrogen to retain the portion corresponding to so much as half of the initial velocity), then such more condensed erupted matter would pass away from the Sun's rule never to return.

The question may suggest itself, however, whether the eruption witnessed by Prof. Young might not have been a wholly exceptional phenomenon, and so the inference respecting the possible extrusion of matter from the Sun's globe be admissible only as relating to occasions few and far between. Now on this point I would remark, in the first place, that an eruption very much less noteworthy would fairly authorise the inference that matter had been ejected from the Sun. I can scarcely conceive that the eruptions witnessed quite frequently by Respighi, Secchi, and Young—such eruptions as suffice to carry hydrogen 80,000 or 100,000 miles from the Sun's surface—can be accounted for without admitting a velocity of outrush exceeding considerably the 379 miles per second necessary for the actual *rejection* of matter from the Sun. But apart from this it should be remembered that we see only those prominences which happen to lie round the rim of the Sun's visible disk, and that thus many mighty eruptions must escape our notice even though we could keep a continual watch upon the whole circle of the sierra and prominences (which unfortunately is very far from being the case).

It is worthy of notice that the great outrush witnessed by Prof. Young was not accompanied by any marked signs of mag-

netic disturbance. Five hours later, however, a magnetic storm began suddenly, which lasted for more than a day; and on the evening of September 7 there was a display of the aurora borealis. Whether the occurrence of these signs of magnetic disturbance were associated with the appearance (on the visible half of the Sun) of the great spot which was approaching or crossing the eastern limb at the time of Young's observation, cannot at present be determined.

I would remark, however, that so far as is yet known the disturbance of terrestrial magnetism by solar influences would appear to depend on the condition of the photosphere, and therefore to be only associated with the occurrence of great eruptions in so far as these affect the condition of the photosphere. In this case an eruption occurring close by the limb could not be expected to exercise any great influence on the Earth's magnetism; and if the scene of the eruption were beyond the limb, however slightly, we could not expect any magnetic disturbance at all, though the phenomena of eruption might be extremely magnificent.

In this connection I venture to quote from a letter addressed to me by Sir J. Herschel in March 1871 (a few weeks only before his lamented decease). The letter bears throughout on the subject of this paper, and therefore I quote more than relates to the association between terrestrial magnetism and disturbances of the solar photosphere.

After referring to Mr. Brothers' photograph of the corona (and remarking by the way that "the corona is certainly *extra*-atmospheric and *ultra*-lunar"), Sir John Herschel proceeds thus:—

"I can very well conceive great outbursts of vaporous matter from below the photosphere, and can admit at least the possibility of such vapour being tossed up to very great heights; but I am hardly yet exalted to such a point as to conceive a positive ejection of erupted particles with a velocity of two or three hundred miles per second. But now the great question of all arises: what *is* the *photosphere?* what are those intensely radiant *things*—scales, flakes, or whatever else they be—which really *do* give out all (or at least $\frac{99}{100}$ths of) the total light and heat of the Sun? and if the prominences &c. be eruptive, why does not the eruptive force scatter upwards and outwards this luminous matter? . . . . Through the kindness of the Kew observers I have had heliographs of the two great outbursting spots which I think I mentioned to you as having been non-existent on the 9th, and in full development on the 10th—both [being] large and conspicuous, and including an area of disturbance at least 2′ (54,000 miles) across. They were both nearly absorbed, or in rapid process of absorption, on the 11th. In my own mind I had set it down as pretty certain that the outbreak must have taken place *very* suddenly at somewhere about the intervening midnight. Well, now! The magnet's declination curves at Kew have been sent me, and, lo! while they had been going on as

smoothly as possible on the 6th, 7th, 8th, and 9th, and up to 11½ P.M. on the latter day (9th), suddenly a great downward jerk in the curve, forming a gap as far as 3½ A.M. on the 10th. Then comparative tranquillity till 11 A.M., and then (corresponding to the re-absorption of the spots) a furious and convulsive state of disturbance extending over the 11th and the greater part of the 12th. I wonder whether anything was shot out of those holes on that occasion! and, if so, what is going on in the inside of the Sun?"

Note on a special point in the determination of the Elements of the Moon's Orbit from Meridional Observations of the Moon. By George Biddell Airy, Astronomer Royal.

The magnitude of the Moon's apparent disk produces a difficulty in the reduction of observations of the Moon which does not occur in the case of the Sun or any of the Planets. In general, the two opposite limbs of the Moon (either those observed in Right Ascension or those observed in North Polar Distance) cannot be fully illuminated, and, in the great majority of instances, they cannot even be in a state approximating to that of full illumination; no attempt can be made to observe the centre; and therefore the only way of determining the apparent place of the centre is, to observe the fully illuminated limb, and to apply to the result a numerical correction representing the computed value of apparent semidiameter at the time of observation. This apparent semidiameter must be computed from a mean semidiameter inferred from observations of both limbs, when both limbs are so near to the state of full illumination, that the small correction depending on the deficiency of illumination of one limb can be calculated with great accuracy. Very few observatories furnish observations adapted to this purpose; and in consequence there will always be a little uncertainty about the value of the apparent semidiameter.

In correcting the observed Right Ascension of a limb, there will therefore always be a small uncertainty on the inferred Right Ascension of the centre, which has one sign before Full Moon and the opposite sign after Full Moon. It is well known that this introduces uncertainty with regard to the coefficients of the two important equations designated the "Variation" and the "Parallactic Equation." These effects, however, are sufficiently well known, and I shall not delay further on them.

The point to which I invite the attention of the Society is one which I believe has never been noticed, namely, the effect of the uncertainty of correction for semidiameter on the inferred results for North Polar Distance of the Moon's centre. I believe that it is not generally known to the investigators of Lunar Theory and

the constructors of Lunar Tables (it was not known to me till I had been a practical astronomer of some years' standing), that near the summer solstice, or (roughly) from the beginning of May to the end of July, only the North Limb of the Moon can be observed; and near the winter solstice, or from the beginning of November to the end of January, only the South Limb can be observed. At other seasons of the year, the north-limb-illuminations and the south-limb-illuminations are mingled in a singular way. I propose now to trace the general effect of the uncertainty in question, as limited in its application by the peculiarity of observation which I have described.

Suppose that a semidiametral correction larger than that which is just is applied to all observations of North Polar Distance. When the north limb is observed, this will increase the inferred North Polar Distance of the centre, and this applies to the summer observations. When the south limb is observed, the supposed too-large correction will diminish the inferred North Polar Distance of the centre; and this applies to the winter observations. Therefore the plane of the Moon's orbit is made to approach too near to the plane of the terrestrial equator. When the Moon's ascending node is at the first point of *Aries*, the inclination of her orbit to the ecliptic is thus diminished; and when the ascending node is at the first point of *Libra*, this inclination to the ecliptic is increased. Thus there is made to appear, in the value of the Inclination of the Moon's Orbit, a periodical change whose period is 19·2 years (the same as the period of nutation). The same result, but with opposite sign, occurs if the employed semidiametral correction is too small.

I have made great efforts to reduce the extent of this uncertainty. Since I have had the command of an Observatory, I have always carefully prepared the plan of observations of each following year before the end of the preceding year. In doing this at the Royal Observatory, a calculation is made for every day on which the Moon can be observed on the meridian, showing the state of the Moon's illumination; and from this, the possibility of using double observations of the limbs for the correction of semidiameters is immediately seen. The number available in Right Ascension is very small; the number of those which can be used in North Polar Distance is larger. I have laid down the rule, that the observations to be employed shall only be those of the gibbous Moon, and that the defect of illumination upon the imperfect limb of the Moon shall not exceed three degrees of the Moon's surface. The results of these are given in the annual volumes of *Greenwich Observations*, and it will be seen there that a considerable amount of information on this important element has been collected.

In my discussion of the Corrections to the Elements of the Moon's Orbit, as inferred from the observations made at Greenwich from 1750 to 1830, printed in Volume XVII. of the Society's *Memoirs*, I have found on page 54, as one of the results obtained

by examination of the observations of N.P.D., an inequality in the inclination expressed by

$$-2''{\cdot}02 \times \text{sine longitude of node.}$$

And in the extension of this discussion, including the results of observations from 1750 to 1851, printed in Volume XXIX. of the *Memoirs*, I have found on page 21,

$$-1''{\cdot}87 \times \text{sine longitude of node.}$$

It is very probable that these terms may have originated, at least in part, in erroneous value of the semidiameter employed in the reductions.

It is proper to remark that any important error in refraction, applying with different magnitudes to the summer observations and the winter observations, will mix its effects with those of erroneous semidiameter.

Royal Observatory, Greenwich,
1871, *November* 15.

On the Geodesic Lines on an Ellipsoid. By Prof. Cayley.
(Abstract.)

The fundamental equations in regard to the geodesic lines on an ellipsoid were established by Jacobi, viz., representing by a, b, c, the squares of the semiaxes, that is, taking the ellipsoid to be

$$\frac{x^2}{a} + \frac{y^2}{b} + \frac{z^2}{c} = 1,$$

(where $a > b > c$), if we introduce the elliptic co-ordinates h, k, and write

$$\frac{x^2}{a+h} + \frac{y^2}{b+h} + \frac{z^2}{c+h} = 1,$$

$$\frac{x^2}{a+k} + \frac{y^2}{b+k} + \frac{z^2}{c+k} = 1,$$

or, what is the same thing,

$$x^2 = \frac{a(a+h)(a+k)}{(a-b)(a-c)},$$

$$y^2 = \frac{b(b+h)(b+k)}{(b-c)(b-a)},$$

$$z^2 = \frac{c(c+h)(c+k)}{(c-a)(c-b)},$$

then, if β be an arbitrary constant, the differential equation of a geodesic line is

$$(1).\quad \text{Const.} = \int dh \sqrt{\frac{h}{(a+h)(b+h)(c+h)(\beta+h)}} + \int dk \sqrt{\frac{k}{(a+k)(b+k)(c+k)(\beta+k)}},$$

and the expression for the length of any arc of the curve is given by

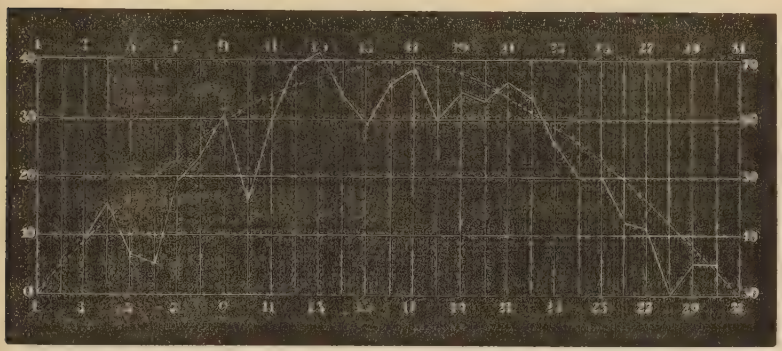
$$(2).\quad s = \int dh \sqrt{\frac{h\,(\beta+h)}{(a+h)(b+h)(c+h)}} + \int dk \sqrt{\frac{k\,(\beta+k)}{(a+k)(b+k)(c+k)}}.$$

I propose in the present memoir to develope the theory to the extent of showing how we can, by means of the first of these equations, explain the course of the geodesic lines; and for given numerical values of a, b, c, calculate, construct, and exhibit in a drawing, the course of these lines. I attend more particularly to the series of geodesic lines through an umbilicus (which lines pass also through the opposite umbilicus) and to the case where the semiaxes are connected by the equation $ac - b^2 = 0$, a relation which simplifies the formulæ.

On the Floor of Plato. By W. R. Birt, Esq.

I have just completed an examination of 133 observations of the tints of the floor of *Plato* by different observers, which has yielded the accompanying curve, and thinking it might be interesting to the Society, I have the pleasure of forwarding it for communication. The observations have been made in accordance with the annexed Form, a light tint having a value of 0·33, medium 0·50, and dark 0·66, higher or lower values have been used to express a very dark or a very light floor. It has long been known that the grey plains appear darkest under a high Sun, but I am not aware that any attempt has hitherto been made to exhibit graphically the progression of tint, and to show its connection with that of the Sun's altitude, which the two curves do unmistakably. The dotted curve is that of the Sun's altitude on the parallel of 50° at the equinoxes, the continuous curve that of the progression of tint, the inflexions of which indicate rather considerable variability, especially in the deepening of the tint. One remarkable feature is, that the chromatic curve lies almost wholly within that of altitude, from which it may be inferred that the full effect of the Sun's influence is scarcely attained, and that the average state of the floor of *Plato* is somewhat *lighter* than it

would be under solar influence alone. It is desirable that observations of this kind should be multiplied, but, from the nature



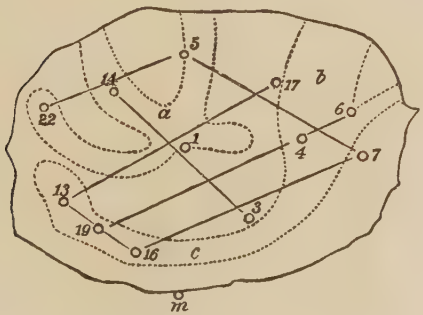
of the investigation, some years must elapse before our present vague and indefinite knowledge can become certain.

Cynthia Villa, Walthamstow, November 25, 1871.

Lunar Map VII.

Plato.

Spots and markings on floor, 1869, August 26.—H. Pratt.



Instructions.—*First*, note the appearance of the border and objects immediately contiguous, if sharp and well-defined, or indistinct and hazy; also the tint of floor, light, medium, or dark. *Second*, record particularly the relative brightness and definition of Nos. 1, 4, 3, and 17, and whether they appear as craterlets or spots. *Third*, give the relative brightness of the remaining spots observed. If a spot be unobserved, write against it "not seen."

Observations. Date

Instrument Power

Border

Floor

No. 1.
,, 3.
,, 4.
,, 5.
,, 6.
,, 7.
*,, 13.
,, 14.
,, 14.
,, 16.
,, 17.
*,, 19.
*,, 22.

* These spots should be particularly sought for.

Remarks

Observer

Lunar Occultations and Eclipses of Jupiter's Satellites.

By John Tebbutt, Esq.

I herewith send a list of lunar occultations and eclipses of *Jupiter's* satellites observed by me at Windsor. The first-mentioned phenomena were not previously computed, so that I was unable, except in two or three instances, to observe the reappearances. A few only of the stars are to be found in the Occultation-List of the *Nautical Almanac*, or in the Catalogues in my possession; but the magnitude of each star and its roughly estimated position, when in contact with the Moon's limb, are given in order to facilitate its future identification. With the exception of the observation of May 18, 1864, all the occultations were observed with my 4-feet refractor and a power of 30. The eclipses of *Jupiter's* satellites were observed with the same telescope, but with a power of 120. The chronometer on the 12th April and 18th May, 1864, was regulated from sextant observations, but on all other occasions with the transit-instrument. It is hoped that the list of occultations may hereafter prove useful for the accurate determination of the longitude of the Observatory, and hence serve for a verification of that of the Sydney Observatory. In conclusion, I may state that the latitude of my Observatory, as finally deduced from a large number of prime vertical observations of stars whose positions have been accurately determined in southern observatories, is $-33^\circ\ 36'\ 29''{\cdot}2$.

Paramatta, N.S. Wales, Sept. 26th, 1871.

Occultations, 1864–70.

Date of Obs.	Mag. of Stars.	Disappearance or Reappearance.	Windsor Mean Time. h m s	Angle from Moon's N. Cusp. °	Remarks.
1864.					
April 12	4½	D	8 46 11·6	80 E	Excellent observation. Disappearance sudden.
May 18	1	D	13 39 16·5	—	Disappearance instantaneous. Obs. made with telescope of 1⅝-in. aperture.
1866.					
April 18	6½	D	6 14 36·2	70 E	Obs. uncertain to 1ˢ, star being faint.
May 17	6½	D	7 2 12·2	80 E	Star appeared to encroach slightly on disk before extinction.
19	7½	D	6 22 52·8	120 E	Star very faint at disappearance.
19	6½	D	6 52 49·8	40 E	Star distinct, but telescope tremulous.
19	8	D	8 48 39·8	70 E	Star distinct.
22	6½	D	8 13 50·2	75 E	Good observation.
Sept. 24	7	D	11 31 7·7	—	Extinction remarkably sudden, being observed near N. limb during total eclipse.
Oct. 15	6½	D	7 21 22·3	100 E	Disappearance instantaneous.
1867.					
May 9	6½	D	6 31 38·4	90 E	Disappearance not quite instantaneous.
Oct. 1	6½	D	7 27 51·4	90 E	Star rather faint.
1868.					
Feb. 27	5½	D	7 48 46·9	80 E	Good observation.
March 2	5	D	10 11 27·2	30 E	Good observation. Disappearance gradual.
May 26	7	D	7 37 36·1	150 E	
26	8	D	7 48 57·6	135 E	Star rather faint.
26	8	D	7 53 31·6	60 E	Star rendered faint by cloud.
27	5	D	7 35 1·6	135 E	
27	7	D	7 42 13·9	110 E	Moon's limb exceedingly distinct and well defined.
27	7½	D	7 45 32·9	90 E	
27	7½	D	7 57 54·9	135 E	
29	7	D	7 0 0·7	90 E	Observation uncertain owing to faintness of star on limb.
June 1	6	D	6 35 14·4	30 E	Disappearance rather gradual.
1	6	D	7 24 44·4	130 E	Ditto more sudden.
Aug. 24	7	D	6 49 54·8	100 E	Good observation.
24	7	D	7 20 56·1	85 E	Good observation.
27	7	D	6 38 43·4	90 E	Star faint at disappearance.
27	6½	D	6 46 14·0	70 E	
27	6½	D	7 25 8·8	60 E	Disappearance uncertain to 1ˢ.
27	6½	D	7 36 19·8	115 E	Disappearance gradual.
27	6½	D	7 52 29·3	120 E	Disappearance sudden.
27	7	D	7 56 20·0	120 E	Disappearance rather sudden.
27	6	D	8 22 19·3	25 E	Disappearance uncertain owing to thin cloud.
28	5½	D	8 1 37·7	70 E	Star probably disappeared about a quarter of a second earlier than time noted.
28	6	D	9 26 52·6	90 E	Obs. 4 or 5 seconds late owing to temporary removal of eye from telescope.
28	6	D	9 40 16·0	160 E	Disappearance gradual and star faint.
Sept. 22	6½	D	7 21 34·1	60 E	Good obs.; disappearance sudden.
26	6	D	8 3 41·2	90 E	Disappearance gradual.

Date of Obs.	Mag. of Star.	Disappearance or Reappearance.	Windsor Mean Time.	Angle from Moon's N. Cusp.	Remarks.
1868.			h m s	°	
Oct. 22	7	D	6 54 50·7	135 E	Star clung to limb for some time.
22	7	D	7 9 33·9	130 E	Disappearance rather gradual.
22	6	D	8 12 7·5	150 E	Ditto.
22	5½	D	8 13 22·0	65 E	Ditto.
22	5½	D	8 27 18·9	75 E	Disappearance pretty sudden.
27	6½	D	9 39 49·0	140 E	Star faint; disappearance rather gradual.
1869.					
Jan. 20	7	D	8 30 56·6	90 E	Star rather faint at disappearance.
Feb. 17	4	D	8 3 32·6	80 E	Disappearance sudden; very good obs.
24	4	D	8 0 49·3	55 E	Good observation; disappearance sudden.
Mar. 22	5	D	10 2 5·0	70 E	Very good observation; disappearance very sudden.
Apr. 19	5	D	8 10 59·5	70 E	
19	6	D	8 55 56·4	70 E	
Nov. 9	5½	D	9 6 52·0	80 E	Disappearance sudden, but followed by a faint flash of light.
Nov. 9	6½	D	9 46 45·8	135 E	Star faint, but disappearance sudden.
Dec. 6	7	D	7 43 56·1	25 E	Star faint; disappearance gradual.
6	7	D	8 2 12·0	65 E	Star rather faint; disappearance sudden.
6	7	D	8 46 20·4	110 E	Star faint; disappearance uncertain to a second.
10	6½	D	8 37 45·4	70 E	Disappearance not quite instantaneous.
1870.					
Jan. 17	7	D	11 59 24·7	—	Disappearance instantaneous.
17	–	D	12 49½	—	Faint star.
17	8	D	13 2 19·3	—	Star faint; time somewhat uncertain.
17	8	D	13 5 0·2	—	Ditto.
17	8	R	13 6 18·7	—	
17	7	R	13 25 4·2	—	Recorded time rather late.
17	7	D	13 36 24·2	—	Time uncertain to a second. All the occultations were observed during total eclipse.
Feb. 8	5	D	9 9 36·3	60 E	Disappearance sudden; observation very exact.
11	3½	D	8 18 1·5	75 E	Disappearance instantaneous.
May 7	7½	D	6 1 58·3	70 E	Disappearance rather sudden.
June 3	5	D	7 19 52·0	70 E	Excellent observation; disappearance instantaneous.
9	6½	D	6 43 38·4	25 E	Good obs.; disappearance rather gradual.
July 4	8	D	6 22 51·9	160 E	Star faint; disappearance gradual. Time uncertain to half a second.
Oct. 1	7½	D	7 40 14·1	110 E	Star faint at disappearance.
1	7	D	7 42 23·6	100 E	Ditto.
1	5	D	7 58 18·0	15 E	Disappearance instantaneous. Faint at reappearance and time probably 2 or 3 seconds late.
1	5	R	8 11 35·5	—	
28	6½	D	8 46 58·8	135 E	Disappearance instantaneous.
29	6	D	7 19 45·3	145 E	Good observations, and disappearances instantaneous.
29	6	D	8 52 45·4	140 E	
Nov. 28	7	D	8 36 51·7	40 E	Star rather faint at disappearance.
30	4	D	10 7 52·2	80 E	Observation uncertain owing to a passing cloud.

Eclipses of Jupiter's Satellites, 1868–70.

Date of Obs.	Satellite.	Disappearance or Reappearance.	Windsor Mean Time.	Remarks.
1868.			h m s	
Aug. 25	II	D	11 17 28·8	Images steady and well defined. Moon present.
Oct. 22	I	R	8 44 16·5	Images tremulous and ill defined.
Nov. 7	I	R	7 4 33·1	Images rather tremulous; twilight had scarcely disappeared.
Dec. 10	III	D	9 39 2·4	Definition pretty good; sky rather hazy. Disappearance very gradual.
17	II	R	7 12 36·5	Strong twilight. Sun's upper limb disappeared on horizon 10 minutes previously. Images tremulous, but pretty well defined.
1869.				
Jan. 25	II	R	9 32 54·0	Planet only about 5° above horizon; images unsteady and ill defined.
Nov. 26	I	R	8 48 34·4	Images well defined; recorded time somewhat late.
1870.				
Jan. 1	III	D	11 8 42·1	Planet very ill defined and boiling.
Feb. 3	I	R	9 38 0·3	Limb boiling.
Dec. 6	I	D	11 56 41·2	Excellent observation.

Reply to the Notes and Queries made by the Astronomer Royal, on the "Observations of η Argûs and its Nebula," Monthly Notices R. A. S. for June 9, 1871, pages 233 and 234. By F. Abbott, Esq.

Note No. 1.—"See in particular the line of four stars convex towards *η Argûs*."

These stars have retained their apparent positions, &c. more than any others in the same object, although variations to a small extent have taken place. In carefully looking over the drawings it will be found that a similarity exists in the position of many of the stars, but as a rule some changes have taken place.

In regard to the use of geometry, it was never my intention to apply it, but only to sketch the object as an eye-draft; and by adopting the term "line of sight," it was intended to simplify, by always observing the object in one position, *i.e.* the telescope lying in a direct line W. N. W. to E. S. E. (the drawing being reversed) and at an angle of from 70° to 80° above the horizon, according to the state of the atmosphere at the time. *In this line of sight* all the drawings have been made; and in an easy, sitting position, with the light shaded from everything but the paper, the object has always been carefully delineated.

With regard to "all the stars agreeing with either Sir John, or Lieut. A. Herschel's configurations," I never expected they

would, as apparent changes have more or less constantly been taking place.

The question asked in No. 7—"The drawing (Lieut. Herschel's) had undoubtedly reached Australia, has Mr. Abbott copied it?"—I answer, No!—The whole of my drawings were made previous to, and independent of, any others.

In making comparison, it would be desirable to refer to the original drawings, as in the lithographs, which are on a reduced scale, some trifling inaccuracies occur.

Private Observatory, Hobart Town,
5th October, 1871.

I have forwarded by the same mail lithographic copies of the drawings, made by order of the Government, and published in the *Proceedings of the Royal Society of Tasmania*.

Note on Mr. Abbott's imagined Discovery of great Changes in the Argo Nebula. By R. A. Proctor, B.A.

I have received a letter and several papers from Mr. Abbott on the subject of the Nebula in *Argo;* and he appears to desire that I should express an opinion as to the reality of the supposed changes in the Nebula. I have, therefore, re-examined his papers and pictures, to which I had given a careful scrutiny two or three years ago. I had then been quite perplexed by the arrangement of the stars as compared with the Cape Monograph. But now, after reading Capt. Herschel's Note on the subject in the last Supplementary Number of the *Monthly Notices*, I find not the least difficulty in interpreting Mr. Abbott's pictures. Unquestionably Capt. Herschel is right. Mr. Abbott has supposed the dark spaces (shown in Sir J. Herschel's Monograph) to correspond to the lemniscate, which would unquestionably imply a complete change in the whole aspect of the Nebula. On the scale of Mr. Abbott's drawings the lemniscate would be about 2-5ths of an inch long; it would, in fact, be a minute and scarcely discernible feature. Mr. Abbott's field of view has a diameter exceeding the length of the rectangular space included in Sir J. Herschel's Monograph. The case is precisely as though an observer with a small telescope, using a power so low as to give a field $1^\circ 8'$ in diameter, were to regard the appearance of the Nebula so seen as a view of the region immediately surrounding the trapezium. It is most unfortunate that by this mistake Mr. Abbott has caused so much valuable time to be wasted by Sir J. Herschel, the Astronomer Royal, Mr. Lassell, and others.

On the Spectrum of Hydrogen at Low Pressure.

By G. M. Seabroke, Esq.

During the late summer months I have been comparing the lines given by hydrogen in the spectroscope with the lines of the solar spectrum, for the purpose of ascertaining whether any lines in the Sun's chromosphere were due to hydrogen, besides those usually supposed to be due to this element. The observations are as yet incomplete ; but as it will be some months before I can again proceed, I therefore produce the results obtained up to the present time. The experiments have been conducted in a room adjoining the Temple Observatory lately erected at Rugby. My mode of proceeding has been briefly as follows :—I use a vacuum tube containing hydrogen, and connected with a Sprengel's air-pump. The tube is of the ordinary form, having the part between the bulbs, into which the platinum wires pass, about $\frac{1}{40}$-inch internal diameter. The pressure in the tube varied from 3 to 4 mm of mercury. Preliminary experiments showed that at this pressure the lines appeared most distinct ; but a slight change of pressure near 4 mm made little alteration in the lines. There is a battery of 12 Smees to work the coil for passing the spark in the vacuum tube. The light from the hydrogen tube passed through a lens which concentrates it on the slit of the spectroscope. A dispersive power of 4 prisms of 60° was used, the arrangement of the instrument being such that the ray of light traverses each prism twice. The room is kept perfectly dark, and sunlight is reflected down from the roof by means of a heliostat. At first I tried the usual mode of comparing spectra, viz. by having the hydrogen and solar spectra side by side. I found this answer very well for the bright lines, but the faint ones could not be distinguished by the side of the bright solar spectrum. I therefore placed a very fine platinum wire in the eyepiece of the spectroscope, and brought the lines under examination into coincidence with the wire, and then passed the sunlight in, and found which black line coincided with the wire, or, where there was no coincidence, the position of the wire with respect to the black lines. I have made from 10 to 20 observations on each line that I have at present examined. I have every reason to believe that the limit of error is within two divisions of Fraunhofer's scale either way. The table below gives the positions of the lines I have already compared. These I hope to examine again next year, and also to finish the remainder.

Reference No.	Position on Kirchhoff's Scale.	Relative Brightness. 10=brightest.	Remarks.
1	694	10	C.
2	881	..	Limit of a number of close lines towards C.
3	930	5	Brightest red line, except C.
4	1014	5	Suspiciously near the chromosphere line near D.

Reference No.	Position on Kirchhoff's Scale.	Relative Brightness. 10=brightest.	Remarks.
5	1049	3	The positions of these were taken by reference to the mercury lines, and are therefore not so reliable as the others.
6	1061	4	
7	1119	3	
8	1533	4	
9	1621	4	
10	1876	4	
11	1943	3	
12	1991	6	
13	2065	3	
14	2080	10	F.
15	2235	4	Near here, exact place uncertain.
16	2361	6	
17	2428·5	..	Limit of a band towards F.
18	2540	2	
19	2605	3	
20	2670	3	
21	2767	..	Faint band.

On comparing the above table with the catalogue of chromospheric lines by Professor Young, published in the *Philosophical Magazine* of November, 1871, I see no sufficient signs of coincidence to lead me to believe that any of the chromospheric lines in his list are due to hydrogen, except the C and F, already well known to be so due. Since I have not yet examined lines further than 2767, the "near G" (2796) and *h* lines, also known to be due to hydrogen, are not mentioned in the above list. In these experiments a spectroscope with a large dispersive and magnifying power was found to be required in order to identify the lines in the solar spectrum, so that the hydrogen spectrum became so reduced in brightness that, in order to see the fainter lines, the eye required to be kept for some minutes in the dark room, although with a spectroscope of low power the spectrum appeared very bright and full of lines.

Occultation of ε Capricorni by the Moon, Nov. 18th, 1871, observed at Forest Lodge, Maresfield. By Capt. William Noble.

The star disappeared instantaneously at the

Moon's dark limb at $22^h\ 52^m\ 31^s{\cdot}3$ L.S.T. $= 7^h\ 3^m\ 0^s{\cdot}7$ L.M.T.

and reappeared, pretty sharply, at the

Bright limb about $23^h\ 39^m\ 8^s{\cdot}0$ L.S.T. $= 7^h\ 49^m\ 58^s{\cdot}9$ L.M.T.

The Moon was low, and there was a good deal of undulation. Power 255, adjusted on the star.

An Eclipse of Jupiter's 3rd Satellite, December 4, 1871.
By Capt. William Noble.

It is impossible to say at what instant the satellite first commenced to diminish in brightness; but it certainly began to wane some minutes before its final extinction. I got my last reliable glimpse of it at $8^h\ 52^m\ 16^s{\cdot}6$ L.M.T., which I assume to be $= 8^h\ 51^m\ 59^s{\cdot}1$ G.M.T. Its disappearance, according to the *Nautical Almanac*, should have taken place at $8^h\ 50^m\ 40^s{\cdot}4$ G.M.T.; showing an error of $1^m\ 18^s{\cdot}7$. I confess, however, my ignorance as to whether the final extinction of a satellite, the instant when it is theoretically dichotomized,—or what?—is spoken of as an eclipse, in our National Ephemeris.

Occultation of the Planet Vesta by the Moon on Dec. 30, 1871.
By Mr. Hind.

The planet *Vesta* will be visibly occulted by the Moon in these latitudes on the evening of December 30. Though the phenomenon is one merely of astronomical curiosity, I have calculated the circumstances for Greenwich, and find them as follow:—

		h m	°
Immersion	Dec. 30	10 44 M.T.	88
Emersion	,,	11 52	241

I am not aware that there is upon record any observed occultation of this planet, or of any other member of the group. The brightness of *Vesta* will render the observation an easy one, in good telescopes.

Observations of Encke's Comet. By H. W. Hollis, Esq.

I note that, at the last meeting, certain remarks were made by the Astronomer Royal and others upon the appearance of Encke's Comet. I have paid considerable attention to this object by means of my 8-inch achromatic, and as I have not yet seen any report which agrees with my own observations, I beg to lay before the Society a drawing copied from one made by myself at the telescope on the evening of the 12th November last, with a Kellner eye-piece, power 120. The drawing is carefully made, and I consider truly represents what I saw. My attention was particularly arrested by the sharp definition of the two edges of the fan-shaped nebulosity; and I could not help asking myself whether the true form of the body were not in all probability a hollow cone. I say a *hollow* cone, because I distinctly saw by averted vision a slight condensation of the light towards both edges, particularly the one running N. and S., on the following, or eastern, side of the comet, whereas a solid cone would appear brightest in the middle and grow fainter to the edges. I made an attempt to

measure with the position-micrometer the angle formed by the two well-defined edges, but the comet would not bear any illumination of the wires, so I inserted a thick wire (made of a horse-hair) which I could see plainly against the sky, and by this I made the angle 86 degrees ±. Definition was exceptionally good, the next entry in my observatory-book being "δ *Cygni* clearly split."

Keele, Newcastle, Staffordshire, Dec. 2, 1871.

Observations of Tempel's Comet (1871, *Nov.* 3), *at Mr. Bishop's Observatory, Twickenham.* By J. R. Hind, F.R.S.

	Twickenham M.T.	R.A.	N.P.D.
	h m s	h m s	° ′ ″
Nov. 8	5 47 34	18 39 50·07	104 9 15·9
9	6 9 24	18 40 17·80	105 7 40·8
10	5 51 29	18 40 43·95	106 4 19·1
12	5 52 38	18 41 38·70	107 57 24·7

Post-perihelion Places of Tempel's Comet, calculated from Mr. Hind's Elements. By Mr. W. E. Plummer.

0^h G.M.T.	R.A.	N.P.D.	Log. Δ.
	° ′	° ′	
1871, Dec. 21	281 50	139 55	0·1340
29	280 6	145 30	0·1142
1872, Jan. 6	277 23	151 21	0·0786
14	273 2	157 20	0·0342
22	264 14	164 8	9·9795
30	233 30	171 28	9·9171
Feb. 7	151 8	168 55	9·8546
15	126 16	153 7	9·8108
23	119 40	133 24	9·8105
Mar. 2	117 22	115 1	9·8597
10	116 46	100 59	9·9370
18	117 4	91 11	0·0199

On Feb. 15 the intensity of light is three times that at the date of the Comet's discovery, and on March 10 it is precisely the same. The Comet may be again observed in Europe in March.

Elements and Ephemeris of Tempel's Comet.
By J. R. Hind, F.R.S.

The following elements are founded upon an observation by Dr. Winnecke at Carlsruhe on Nov. 5, and two observations at Mr. Bishop's Observatory on Nov. 8 and 10.

Perihelion Passage, 1871, Dec. 20·29115 G.M.T.

Longitude of Perihelion ..... 263° 33′ 52″ } Apparent Equinox,
,, Ascending Node.. 147 28 38 } Nov. 0.
,, Inclination 82 31 21

Log. Perihelion Distance 9·8321610.

Motion retrograde.

For the middle observation $\Delta\alpha\,(c-o) = +7''$, $\Delta\alpha\,.\cos.\delta = +11''$.

Hence the following Ephemeris, which may be useful in the reduction of the observations :—

0^h G.M.T.	R.A. h m s	N.P.D. ° ′	Log. Δ.
Nov. 3	18 37 44	98 57·3	0·10155
4	38 7	99 58·0	0·10355
5	38 30	100 58·0	0·10516
6	38 54	101 57·6	0·10698
7	39 19	102 56·6	0·10881
8	39 45	103 55·1	0·11066
9	40 11	104 53·0	0·11253
10	40 38	105 50·5	0·11438
11	41 5	106 47·4	0·11621
12	41 32	107 43·8	0·11803
13	42 0	108 39·7	0·11985
14	42 28	100 35·1	0·12167
15	18 42 57	110 29·9	0·12349

The co-ordinate constants, for the equators, are,

$$x = r.\,[9{\cdot}9274093]\,.\,\sin(v + 21^\circ\,20'\,37'')$$
$$y = r.\,[9{\cdot}7590534]\,.\,\sin(v + 175\ 17\ 16)$$
$$z = r.\,[9{\cdot}9898798]\,.\,\sin(v + 283\ 26\ 2)$$

Ephemeris of Tuttle's Comet for the Southern Hemisphere.
By J. R. Hind, F.R.S.

The following Ephemeris is calculated from the elements of Dr. Tischler, but with perihelion passage taken Dec. 1·7974 G.M.T. When found by Dr. Winnecke on October 14, and considered pretty bright, I = 0·58.

0^h G.M.T.	R.A. h m s	N.P.D. ° ′	Log. Δ.	I.
1871, Dec. 18	11 52 2	143 5·5	9·9286	1·24
20	11 58 57	145 20·6	9·9399	
22	12 6 12	147 27·2	9·9511	1·08
24	12 13 48	149 25·7	9·9623	
26	12 21 45	151 16·3	9·9734	0·94
28	12 30 3	152 59·5	9·9843	
30	12 38 42	154 35·6	9·9950	0·82

0h G.M.T.	R.A. h m s	N.P.D. ° ′	Log. Δ.	I.
1872, Jan. 1	12 47 44	156 5·0	0·0054	
3	12 57 9	157 28·0	0·0156	0·72
5	13 6 58	158 45·1	0·0255	
7	13 17 12	159 56·4	0·0349	0·63
9	13 27 50	161 2·3	0·0441	
11	13 38 52	162 3·0	0·0530	0·55
13	13 50 18	162 58·7	0·0616	
15	14 2 7	163 49·8	0·0698	0·48
17	14 14 16	164 36·7	0·0778	
19	14 26 43	165 19·5	0·0854	0·43

Elements of Minor Planet (116). By Dr. R. Luther.

Epoch 1871, September 23h·0 Berlin Mean Time.

L ..	8° 58′ 12″·6	
M..	221 44 19·7	
π ..	147 13 52·9	Mean Equinox, 1871·0.
Ω..	64 14 13·0	
i ..	3 35 28·7	
φ ..	8 31 35·0	
μ ..	764″·812143	
Log. *a*	0·4443012	

These elements have been computed from observations made in Germany.

Comet IV., 1871, *discovered by M. Tempel at Milan on the 3d November.*

The elements, calculated by MM. Oppolzer and Schulhof, are as follows :—

T = December 20·1155 Berlin M.T.

π =	22° 25′ 39″	Mean Equation 1871·0.
Ω =	145 19 53	
i =	102 7 40	

Log *q* = 9·87628.

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